gested, thickened and adherent to the intestines, which in many places could not be separated from each other, but with considerable difficulty.

The portion of the intestine removed at the time of the injury was found to be the colon: it having been divided about four inches from the entrance of the small intestine. The divided portions were drawn together at the place of injury and united by organized lymph, which also connected the intestines to the parietes of the abdomen where the wound was made. The passage between the divided ends of the intestine was small and crossed by a few ligamentous-like bands—but still large enough to permit the passage of semi-liquid feces.

Judging from the size of the intestine removed, which was diminished by being drawn out at a small opening, we had erroneously supposed, without particular examination, that it was a portion of the small intestine, and so stated in our former communication.

UTICA, *Nov.* 15, 1845.

ART. V.—*On the Contagiousness of Puerperal Fever.* By SAMUEL KNEELAND, JR., M. D., of Boston.

HAVING arrived, often after numerous inconveniences and even dangers, at the ordinary term of pregnancy, many women, at the moment of enjoying the "sweet prerogative" of being mothers, are still far from having nothing to fear from the interesting and painful function which they have fulfilled. It is but too common to see them, at this period, fall the helpless victims of a complicated, fearful disease; which, from its being developed in the puerperal state, has received the name of puerperal fever. Of so fatal a nature is this affection, that, in some of the epidemics of Paris, London, Dublin and Edinburgh, hardly one in thirty of those seized with it recovered. To combat this Protean disease, all the resources of medicine have been often tried in vain; the very multiplicity of the means employed attests their want of success.

But, it may be asked, what is this disease known by the name of puerperal fever?

We shall not here enter into the consideration of the various theories which have been entertained on this subject; these will find a more appropriate place hereafter. Some have objected to the term "puerperal fever," as conveying no idea to the minds of those not familiar with the disease; indeed, since morbid anatomy has of late years so absorbed the attention of physicians, constant attempts have been made to localize the disease in question. Having been supposed, from the time of Hippocrates to that of Boerhaave, to be dependent on an inflammation of the uterus, it was afterwards localized in the peritoneum by Johnston, in England, Walter, in Prussia, and Bichat, in France; and the name of peritonitis was given to it. Then as inflammation of the uterus was found combined with that of the peritoneum, the name was changed to metro-peritonitis; metritis was the next appellation; then uterine phlebitis, pus having been found in the veins of the uterus; as this was sometimes found only in the lymphatics, it was hence called lymphangitis; others, especially the Germans, (with Boër at their head,) discarded all these as false, and substituted putrescence or softening of the uterus; this again did not satisfy the French pathologists, who, having met with pus in the cellular tissue, in the viscera, and the articulations, without lesion of the uterus or peritoneum, came to the conclusion that it was a purulent fever, which is the prevailing opinion in the present French school. Thus, by attempting to localize and simplify, this system has only rendered still more confused and difficult this important subject. Each observer has described faithfully his epidemic; each has been so far in the right: but all have been wrong in maintaining that the type of disease observed by them was the true type of puerperal fever, to the exclusion of all others. To avoid this confusion, then, which has a direct bearing on the subject of contagion, we shall consider puerperal fever as a general diseased condition of the system, which may at one time manifest itself by a peritonitis, at another by a phlebitis, &c. The term puerperal fever is, we think, the best, as it conveys a sufficiently clear idea to the minds of all; which could be conveyed by none founded upon the different anatomical lesions found after death—and besides it is not confined to any one theory.

The history of the disease is very imperfect in the old authors; and it is not till the middle of the 17th century, when it occurred as a malignant epidemic at the Hôtel Dieu, of Paris, that it seems to have occupied the attention of physicians to any great extent. Since then, it has continued to rage with more or less severity, so that it has been constantly before the medical world. It is allowed by all that there is an epidemic spread of this affection; but it is not by any means a universal opinion that it may become contagious.

This brings us to the subject of contagion, the most difficult question, perhaps, in medicine; and one which will forever remain a question to those who require mathematical proof before they can yield assent.

There have been usually admitted three causes in accounting for the spread of epidemic diseases in general: some peculiar miasm existing in the atmosphere; some telluric emanation; or effluvia generated in the human body, and capable of exciting disease in a healthy individual. We do not think that any one will maintain that puerperal fever originates from any limited, or diffused telluric emanation, as it is not confined to certain localities, resembling one another in any respect; but, on the contrary, it is met with in districts the most dissimilar in all respects; (excepting that it becomes more rare, as we approach the tropics). We have, then, left the unknown atmospherical condition which causes its epidemic spread and contagion; the former needs no discussion here.

Before proceeding to enumerate the reasons for our belief in the contagious character of this disease, it will not be amiss to state briefly what we mean by the different forms of contagion. Very nice distinctions are to be found in various authors between contagion and infection, which need not be insisted on in treating of an affection where both probably operate; in fact, contagion is used by many as including infection. Dr. Elliotson remarks, that "the word 'contagious' is used in the same way as the term 'horse.' The latter is used to include both a horse and a mare; but it is frequently applied to the male only; and so these diseases are all continually spoken of as contagious; but the word 'contagious' is also employed in a limited sense, to signify those diseases which are communicated by actual contact; or by touching something which the patient has touched; or something which has palpably proceeded from him."

There are, then, two ways in which diseases may be communicated from one individual to another, contagion and infection. When the sick exhale miasmata, which, by traversing the air, excite the same disease in a person before healthy; or when they secrete a matter which will produce the like effect by direct contact either with it, or things impregnated with it; contagion is said to exist—when the same deadly influence is exerted by an infected place upon a mass of sound individuals, infection is said to operate. Contagion supposes, therefore, a disease transmitted from a sick *person* to a sound one; while infection generally arises from animal or vegetable matter in a state of putrefaction. Those contagions, in which the atmosphere is the means of communication, are alone liable to be confounded with infection; and this is precisely the case with puerperal fever. Contagion, when once produced, has no need of the causes which gave it birth; it, in a manner, reproduces itself, and is independent, to a certain extent, of atmospherical conditions: while infection supposes, from its commencement, and during its course, an alteration of the atmosphere; the infected places affect individuals only within their sphere of activity, in whom there may be a predisposition; but these may become so many foci of infection, impregnating the air with miasmata capable of exciting the disease in the predisposed; here, infection becomes a species of contagion. Some diseases are communicable by contagion alone; others by infection alone; others by both contagion and infection: as instances of the three may be mentioned syphilis, hooping-cough, and small-pox—in the latter class we would place puerperal fever. Finally, contagion is indicated by a succession of cases, in narrow limits, and during a considerable period; while, in infection, there is a simultaneous attack of many individuals, and in more extended limits.

We, then, use contagion as a generic term, signifying the transmission of diseases from a sick to a healthy person, both by immediate contact, and by means of the air, or fomites impregnated by the effluvia arising from those affected.

Having premised these general remarks, which the non-contagionists have usually overlooked, we shall now take up the question of the contagiousness of puerperal fever. As it is not the long array of well authenticated facts which alone will carry conviction to the skeptic, it will be our object to present, and, if possible, to invalidate some objections which are always made when the contagiousness of this affection is discussed.

The chief fear of the non-contagionists seems to be, lest, by allowing that it may become contagious under certain circumstances, they be forced to admit that it is an essentially or uniformly contagious disease. This arises from a too prevalent opinion that contagion is a necessary incident of a given disease, that it constitutes its very essence; while, in reality, it is a mere accident: the effluvia, or morbid secretions from a diseased body, which constitute contagion, are not necessarily a poison to all exposed to them; a *predisposition* is necessary. M. Michel, and others have gone to the other extreme, and maintain that any continued fever, under the influence of peculiar individual conditions, and favoured by external circumstances difficult to determine exactly, may become contagious.

Perhaps if all the authors who have written on this subject were arrayed as contagionists, or non-contagionists, the latter would outnumber the former; but even admitting that the majority were to decide a disputed question in medical science, the opinions of the latter require some investigation. Every physician who brings instances of the action of contagion, if only a single authenticated case, makes a real, valuable addition to the mass of facts, while he who cites a hundred instances where the contagion did not take effect, merely adds a negative fact, having no real value in our present question. There may be circumstances of place, intervals of time, and collateral cases, which may well explain the seeming inconsistency, even if there were not another more satisfactory explanation. Many have supposed that, because every one exposed to the contagion is not attacked, therefore the affected are not attacked in consequence of their exposure. This most brilliant method of jumping at conclusions betrays an ignorance, or a neglect of one of the simplest principles in medicine; viz., that, before any exciting cause of disease can exert its morbific influence on the animal economy, there must exist a certain unknown alteration in it, a certain susceptibility, which is called predisposition: were not this predisposition requisite, every one exposed to the exciting causes of a disease should have always that disease, and no other; and every epidemic should have completely depopulated the locality it happened to attack. Like causes produce like effects; but in order that this may hold true, there must be a similarity of circumstances in which the causes are applied. From the complex nature of the structures of the human body, and its various functions, acting and being acted upon by an infinity of agents imperfectly known to us, it must ever be impossible to demonstrate the above principle by an appeal to the senses; both parties must then prove, or disprove, approximatively, that exposure to the contagion is followed by the disease, as the effect follows the cause. If it be argued, then, that, because exposure is not always followed by the disease, the affection is not contagious, the same arguments would equally shake our belief in the contagious character of small-pox, and other diseases admitted by all to possess this character. There is no epidemic which affects every individual placed within its range; no sure contagion attacking all exposed to it. Inoculation from the same pustule of variola, or vaccinia, communicates the disease to one child, and not to another, under circumstances as far as can be determined, perfectly similar; of two persons bitten by a rabid animal, one shall be seized by hydrophobia, and the other escape; cohabitation with individuals affected with the itch and syphilis, is not invariably followed by these diseases; courageous physicians have inoculated themselves with the matter of the plague with impunity, while the poor soldier, less exposed, has contracted it—these singular immunities are common to all contagious diseases. To require, then, before allowing the contagious character of a disease, that its propagation should form a continuous, unbroken chain, is to require an impossibility; to deny its transmissibility, because it is not infinite, is an absurdity.

Another point which has much confused this question is the vague signification of the word "epidemic," which by many is understood as implying some mysterious quality of a disease, in virtue of which it attacks a great number of individuals. This word means "upon" or "among the people;" a disease spreading over a more or less numerous population.—Again, a disease is said to be "epidemic," when its propagation is attributed to a morbific principle contained in the air. Strictly speaking, the term "epidemic" implies a *number* of persons affected, and not a *quality* of a disease. It is by the *number* of patients that such an affection is judged to be *epidemic;* and it is also by the *epidemic quality* attributed to the affection that the *number* of patients is explained—here, then, is a circle of confusion, in which the cause becomes the effect, and the effect the cause, according to the will or caprice of the observer. For those who consider as epidemic diseases those propagated by a morbific atmospheric principle, it will always be easy to make objections to the contagionist, unless the proofs are so convincing as to silence all opposition; which is rarely the case. Whether this double meaning of the word "epidemic" should be done away with, it is not our business to inquire at the present time—for our part, we do not believe that epidemic diseases are always the results of an *atmospheric principle*, but are frequently, as in puerperal fever, the results of *contagion*—we would not think the *epidemic* always the *cause* of the contagion; for, though in an epidemic, the chances of contagion increase in proportion to the number of patients, if we analyze most of the so called epidemics of puerperal fever, we shall see that the order of succession of those attacked shows that the epidemic was the *effect* of the contagion. If this can be shown, one of the strongest arguments of those who maintain that the spread of this affection is always owing to an epidemic influence, will, we think, fall to the ground. We would not be understood as implying that the disease is entirely independent of epidemic and endemic influences; on the contrary, like all other contagious diseases, it has shown such eccentricities in its occurrence and recurrence in various parts of the same district, that it must be allowed that its spread is often due to external agents acting upon many individuals at the same time—in other words, that it is not exclusively propagated by contagion; that an epidemic constitution is necessary to co-operate with the contagion, before it can spread extensively.

Let us now examine some of the best authors, and see if the succession of cases, in a single physician's practice, does not, in the history of almost every epidemic, furnish an immense body of incontrovertible evidence in favour of the contagiousness of puerperal fever.

In most contagious diseases, occurring in large cities, it is difficult to trace the communication from one individual to another, from the continual intercourse carried on; and therefore, it is chiefly the physicians of such large communities who are the strongest opponents of contagion—but in puerperal fever the circumstance of a large population has no relation to the question of contagion, owing to the peculiar state of those exposed to its attacks. When, therefore, we see this disease occurring solely, or chiefly in the practice of a single physician, in a large town, to use the expression of Dr. Blundell, "stalking behind him, wherever he goes, like his evil genius," we are surely justified in attaching some importance to such facts.

This coincidence has been mentioned by most writers on puerperal fever, the opinions of some of whom we shall now mention.

Dr. Gordon, in his account of the epidemic which raged at Aberdeen, in the years of 1789 to '93, says, that he could foretell what woman would be attacked, upon hearing by what midwife they were to be delivered; and that in almost every case his prediction was verified.

Mr. White, of Manchester, observes, that he is acquainted with two physicians who have the whole business of midwifery, in a large town, divided between them; one loses several patients every year by the disease, while the other has not a single case.

Dr. Armstrong in his essay on the Sunderland epidemic, mentions several instances of a similar confinement of the disease to one man's practice.

Drs. Ramsbotham, Lee, Gooch, Roberton, Hutchinson, Blundell, and a host of others, mention the fact of the greater number of observed cases occurring in the practice of single individuals, while their medical brethren had no cases.

Dr. Holmes (in the *N. E. Quarterly Journal of Med. and Surg.*), speaks of a series of cases in an American Journal, observed by Mr. Davies, who says, "in the autumn (1822) he met with twelve cases, while his medical friends in the neighbourhood did not meet with any, or at least very few." He also mentions nine cases occurring in the practice of Dr. Pierson, of Salem, in 1829: "Up to this period," says the latter, "I am not informed that a single case had occurred in the practice of any other physician," admitting, however, that his information may have been defective. In a letter addressed to Dr. Storer, it is stated: "About three years since, a gentleman in extensive midwifery business in a neighbouring state, lost, in the course of a few weeks, eight patients in child-bed, seven of them being undoubted cases of puerperal fever. No other physician of the town lost a single patient of this disease during the same period."

In the *American Journal of the Medical Sciences* (for Oct., 1842, p. 410), is the Quarterly Summary of the Transactions of the College of Physicians of Philadelphia, from which we extract the following: Dr. Condie, having alluded to the prevailing puerperal fever of a peculiarly malignant character, observes that, "in the practice of one gentleman, extensively engaged as an obstetrician, nearly every female he has attended in confinement, during several weeks past, within the above limits, had been attacked by the fever." If it be not contagious, "how otherwise can be explained the very curious circumstance of the disease, in one district, being exclusively confined to the practice of a single physician, a Fellow of this college, extensively engaged in obstetrical practice—while no instance of the disease has occurred in the patients under the care of any other accoucheur practising within the same district. Scarcely a female that has been delivered by this gentleman for weeks past, has escaped an attack."

These, and many other authorities which might be adduced, prove, beyond doubt, the singular confinement of the disease to one man's practice. But how can this fact be explained? We should not wonder at it, if occurring in the pestilential wards of a hospital; but, in private practice it is certainly very strange, that the disease should follow precisely the steps of one unlucky individual, tracing him far and wide with the certainty of a blood-hound. Considering the exceeding rareness of the disease (comparatively) even in private practice, it certainly seems impossible to attribute to mere chance the numerous well-authenticated instances of the above fact; they who can swallow the immense improbability of the doctrine of chance in this matter, need hardly strain so much at the infinitely less improbability of contagion.

Clearly connected with the above fact is the question of its propagation by being carried by physicians and nurses. The whole question turns upon this; for, although the succession of effects to causes does not absolutely prove the dependence of the former on the latter, if it can be shown that the instances of contagion (supposed) occurred very much more frequently, (and in many instances solely,) where there was communication between a physician and those affected, than where there was no such communication, are we not justified, in a measure, in establishing the relation of causation? The question of the contagious nature of a disease, according to Dr. Alison, (and the same reasoning may be applied to all sciences, as well as medicine,) always ultimately turns on a calculation of chances. "The question always comes to this—is the circumstance of intercourse with the sick followed by the appearance of the disease, in a proportion of cases so much greater than any other circumstance common to any portion of the inhabitants of the place under observation, as to make it inconceivable that the succession of cases occurring in persons having that intercourse should have been the result of chance? If so, the inference is unavoidable, that that intercourse must have acted as a cause of the disease. All observations which do not bear strictly on that point are irrelevant, and, in the case of an epidemic first appearing in a town or district, a succession of two cases is sometimes sufficient to furnish evidence, which, on the principle I have stated, is nearly irresistible."

Let us submit this question to the test of Dr. Alison, by quoting the opinions of some of the best authors.

Dr. Gordon says, "The disease seized such women only as were visited, or delivered by a practitioner, or taken care of by a nurse who had previously attended patients affected with the disease. I had evident proofs that every person who had been with a patient in the puerperal fever, became charged with an atmosphere of infection, which was communicated to every pregnant woman who happened to come within its sphere. It is a disagreeable declaration for me to mention, that I myself was the means of carrying the infection to a great number of women."

Dr. Gooch mentions the case of a physician who had several deaths in his practice from this disease in quick succession; when thinking he might have carried the contagion in his clothes, he changed them, and had no more cases.

Dr. Ingleby, in the *Edinburgh Medical and Surgical Journal*, (vol. 49, p. 415,) observes, that Dr. Campbell thinks that, "unless the practitioner has been engaged in the dissection of the bodies of those who have fallen victims, the disease cannot be conveyed by him from females labouring under it to others recently delivered." "But the fact of the extension of the disease in this manner from the living subject has been most satisfactorily shown by Mr. Roberton. I have repeatedly observed the same myself, and have submitted a number of cases in illustration. I have also adduced the strongest ground for believing, that the effluvia derived from the body of a woman who died from puerperal fever were conveyed in this manner by two practitioners, and rapidly produced the same disease in two other females."

Dr. Blundell, Abercrombie, Ramsbotham, and many others, assert that the contagion is often carried about by physicians and nurses, and many of them accuse themselves of having been the vehicles of its conveyance.—When to this testimony is added the hundred fold more which is buried with the hapless victims, we have an amount of facts which it is obstinacy, or willing blindness not to perceive the force of.

But, unfortunately, we have no need of crossing the Atlantic to search for evidence of this fearful truth.

In the *American Journal of the Medical Sciences,* (Oct., 1842,) Dr. West stated some facts communicated to him by Dr. Jackson, of Philadelphia, who, when practising in Northumberland County, had seven cases of delivery in rapid succession, in all of which puerperal fever supervened; of which five proved fatal. "Women," said he, "who had expected me to attend upon them, now becoming alarmed, removed out of my reach, and others sent for a physician residing several miles distant. These women, as well as those attended by midwives, all did well; nor did we hear of any deaths in child-bed within a radius of fifty miles, excepting two, and those I afterwards ascertained to have been caused by other diseases. I now began to be seriously alarmed on the score of contagion. Although I had used some personal precautions before, I now feared that they had not been sufficient."

Dr. Holmes, in the Journal before alluded to, gives a series of cases occurring in the practice of a physician of a town at some distance from Boston. There were seven cases, which happened between the 20th of March and the 8th of May; the first five proved fatal in from four to seven days after delivery; the other two recovered. These were the only cases attended by this physician during the above period; "and no other cases of a similar character with those of Dr. C. occurred in the practice of any of the physicians in the town or vicinity at the time."

In the first letter to Dr. Storer, (in the same article,) is found the following statement: between the 10th and 28th of February, 1830, "I attended six women in labour, all of whom did well, except the last, as also two who were confined March 1st and 5th. Mrs. E., confined Feb. 28th, sickened and died March 8th. The next day, the 9th, I inspected the body, and the night after attended a lady, who sickened and died on the 16th. The 10th, I attended another, Mrs. G., who sickened but recovered. March 16th, I went from Mrs. G.'s room, to attend a Mrs. H., who sickened and died on the 21st. The 17th, I inspected Mrs. B. On the 19th, I went directly from Mrs. H.'s room to attend another lady, who also sickened and died on the 22d." "Up to the 20th of this month I wore the same clothes. I now refused to attend any labour, and did not till April 21st, when having thoroughly cleansed myself, I resumed my practice, and had no more puerperal fever. These cases were not confined to a narrow space. The two nearest were half a mile from each other, and half that distance from my residence. The others were from two to three miles apart, and nearly that distance from my residence. There were no other cases in their immediate vicinity which came to my knowledge."—"Of the six cases you perceive only one recovered."

The successive communication of the disease by single physicians and nurses to their patients cannot always be thus satisfactorily traced; and hence the non-contagionists maintain that the doctrine of exclusive, if not of occasional, contagion must fall to the ground. Without again showing that this negative kind of evidence is of no weight against the more positive kind which has been now brought forward, or that we do not argue for the exclusive contagion of puerperal fever, we shall merely state, for the careful study of those who consider this argument as unanswerable, that it is quite as strong for small-pox and other contagious diseases, in which this communication cannot always be satisfactorily traced. Of a like unsatisfactory and negative character is such reasoning as the following: in the Philadelphia epidemic of 1842, the physician in whose practice most of the cases occurred, says that he cannot easily believe in the transmission of the disease from female to female by a contagion conveyed in the person or clothes of a physician, because, having absented himself from the city, after the occurrence of the disease in his practice, for a week, and on returning, having entirely changed his clothes, his first case of labour was followed by a fatal attack of the fever.

It becomes an interesting question for the conscientious physician, how the contagion is conveyed; as by the knowledge of this he is enabled to regulate the conditions of his intercourse with his patients. Is this disease transmitted by direct inoculation, by the atmosphere carried about by the physician, from patients before death? and is it not also conveyed from the examination of the bodies of the deceased? As to the channel by which the poison enters the system, it is probably both by the vascular and respiratory system in the majority of cases; by the uterine surface and by the lungs. Many have limited its introduction to the "wounded surface" of the uterus—but that the blood is frequently poisoned from its entrance by the lungs, is shown by the fact, that puerperal fever (though the term be misapplied), may seize a woman before delivery; or that the poison may be introduced, and produce the symptoms of this disease, before the local action in the uterus has taken place. Mr. Ingleby says the attack may commence before delivery, and that he has seen a single case. We think we have seen another—a woman entered the Hospital de la Faculté, in the service of M. Paul Dubois, with all the symptoms of puerperal fever of the low type which then existed in the city—she was in the sixth month of her pregnancy, and had never carried a child to the full term—she had the "facies of puerperal fever;" with the abdomen moderately swelled and painful, though not exquisitely so; weak pulse, and great prostration—on examination the os uteri was found dilated, and delivery imminent—a few hours after she was delivered of a child, dead, though not at all putrefied—the symptoms were aggravated after delivery, and death took place in a few hours—at the autopsy, the peritoneum presented evident traces of inflammation, its cavity containing the milky fluid, with membranous flocculi found in the form of the disease, we shall hereafter mention as the erysipelatous; the uterine veins contained pus; and the placental insertion presented that softened, semi-putrid aspect, which the Germans have called "putrescentia uteri."

It probably principally affects the blood, though its first violence may often fall upon the nervous system. It is impossible to determine this point, which is fortunately of secondary importance.

It is, doubtless, very often propagated by direct inoculation from the living subject. Dr. Rigby observes, in his System of Midwifery, "the discharges from a patient under puerperal fever are in the highest degree contagious." "The puerperal abscesses are also contagious, and may be communicated to healthy lying-in women by washing with the same sponge; this fact has been repeatedly proved in the Vienna Hospital."—He also observes, that they are also communicable to unpregnant women; and that frequently abscesses and diffuse inflammation attacked those who washed the bed-linen soiled by the discharges. Of the acrid nature of these, the following case from the *Dublin Journal of Medical Science*, (Nov., 1844,) will afford sufficient illustration. It was here necessary, for the removal of the retained placenta, to pass each arm in succession into the uterine cavity, where they were tightly grasped by the neck. Two days after, pustules appeared on the arms, one of them being surrounded by a livid base, the part of the arm near it becoming hard and swollen.—The writer hence concludes, that a morbid poison was generated epidemically in the blood, contact with which communicated the taint, and converted a common furuncle into a malignant pustule; and this altered condition of the blood was sufficient to produce all the phenomena of puerperal fever in the patient, who was, however, free from the common symptoms of uterine inflammation.

Such being the malignant character of these discharges, that even those who have washed the linen soiled by them, have communicated the disease to others, and that even the simple operation of passing a catheter has been the cause of propagating it, we should naturally expect to find the fluids, after death, of a peculiarly virulent nature. Many cases are on record of the most serious accidents from wounds received in post-mortem examinations of puerperal fever; these cannot be accounted for on the supposition of a common poison acting on an enfeebled constitution, as several cases happen in the course of every epidemic; whereas such cases are comparatively rare in diseases infinitely more common, and in which the same enfeebled condition of the system is always a predisposing cause. From the comparatively greater frequency, then, of serious and even fatal symptoms in autopsies of this affection, we are compelled to recognize the existence of a most deadly and peculiar poison. This is so deadly that no wound is necessary, in order that the fatal effect may take place. Dr. Duncan, in the Transactions of the Medico-Chirurgical Society of Edinburgh, mentions an instance, where Dr. Cumming was present at the dissection of one who died of puerperal fever. "He took no share in the dissection, excepting introducing a fresh thread into the needle which was employed in sewing up the body, and was not aware of any abrasion, or of having punctured himself in the act of threading." In about a week after, he experienced an uneasy sensation in the middle finger of the left hand, where was discovered "an angry pimple"—death took place on the 11th day, with evident signs of a profound alteration of the blood.

With these startling facts before us, we are prepared for the long list of victims to the physician's ignorance and negligence; a list, which we fear is yet to be swelled, before the fearful truth we have endeavoured to bring forward shall be graven on the tablets of Medical Science.

From the mass of authority showing that the contagion of puerperal fever is liable to be conveyed by physicians to their patients, from their being present at, or taking part in the post-mortem examinations in this disease, we shall select enough, we trust, to convince the most skeptical.

In 1821, Dr. Campbell, of Edinburgh, attended the autopsy of a married woman, who died of puerperal fever, after an early abortion; he removed the pelvic viscera and external parts, and carried the whole in his coat pocket to his class room; the next morning, having on the same clothes, he assisted, with some of his pupils, at an instrumental delivery at Bridewell; this woman was attacked with puerperal fever, and died—(in the autumn). The same night, he went with another physician to deliver a woman, who also died; three others shared the same fate. Similar instances occurred in his practice in the summer of 1823: assisting at a dissection of this disease, (at the time having no cases of it,) from the poverty of the people, he could not properly wash his hands; without any farther attention, he went, when he returned home, to two cases of labour; both were seized with the disease, and died. Other physicians of the city, who had similar misfortunes, convinced of the contagious nature of the disease, gave up, for a time, the practice of midwifery.

Dr. Rigby, in the Library of Medicine, (vol. 6,) says that it is highly unsafe for one to attend a case of midwifery after a post-mortem of puerperal fever; and that it is impossible to remove the smell from the hands for several hours, even by frequently repeated washing.

In the British and Foreign Medical Review, (Jan., 1842,) the same author states, in a review of Dr. Kiwisch's work on the diseases of child-bed women, (who does not believe in the contagion of this disease,) that a young physician, contrary to advice, examined the body of a woman who had died from puerperal fever; there was at the time no epidemic; the case seemed to be purely sporadic: three other women were soon after delivered by him, all of whom died of this affection, the symptoms of which broke out soon after delivery. "The patients of his colleague did well, *except one*, where he assisted to remove some coagula from the uterus; she was attacked in the same manner as those whom he had attended, and died also: we trust that this fact alone will forever silence such doubts, and stamp the well-merited epithet of 'criminal,' as above quoted, upon such attempts."

Dr. Gooch remarks: "A practitioner opened the body of a woman who had died of puerperal fever, and continued to wear the same clothes. A lady whom he delivered a few days afterwards was attacked with, and died of a similar disease; two more of his lying-in patients, in rapid succession, met with the same fate."

The following is a still more striking example: Dr. Merriman, as quoted by Dr. Holmes, "related an instance occurring in his own practice, which excites a reasonable suspicion that two lives were sacrificed to a still less dangerous experiment. He was at the examination of a case of puerperal fever at two o'clock in the afternoon. *He took care not to touch the body.* At nine o'clock the same evening he attended a woman in labour; she was so nearly delivered, that he had scarcely anything to do." She died in 48 hours; and the child also died *of erysipelas*, two days afterwards.

In the London Cyclopedia of Practical Medicine are several cases of physicians who had examined the bodies of those affected with this disease, all of whose patients, in quick succession, fell victims to a similar affection.

In the American Journal (above quoted) it is stated that Dr. Warrington examined the body of a woman who died of this disease, and laded out the contents of the abdominal cavity with his hands. A few days after, he was called upon to deliver three women in rapid succession. One was attacked with metritis, another with partial peritonitis; both were very sick, but recovered: in the third case, the patient was seized with peritonitis, and died on the fifth day. Two other women in his practice were also attacked with it, and both died. He would not be present at the autopsies of these cases, for fear of communicating more readily the disease.

Farther proof of its communication in this manner will be adduced hereafter.

Having attempted to show that puerperal fever may be communicated by physicians and nurses, either by direct inoculation of, or by an atmosphere impregnated with the miasmata and fluids of the living and the dead, we would not be understood as implying that all forms are equally contagious, or the same form at different times; from inattention to this we are convinced arises the contradictory testimony of authors, in the different epidemics. That there may be some forms rarely, if ever, contagious, we do not deny; but we are equally convinced that the erysipelatous form (of which more hereafter) is contagious when purely sporadic. When a particular epidemic constitution is prevalent, favourable to the extensive spread of the disease, its strictly contagious character is less distinctly seen, from the operation of other causes which agree better with the laziness or interest of the physician; but when the disease is purely sporadic, if (as we have seen) the cases be confined to the practice of a single physician, there is a strong probability, to say the least, of the existence of a contagious principle; when, moreover, in different countries, and at different times, we see this same fact continually mentioned, the probability approaches so near to certainty as to deserve to influence the conduct of the physician, in his intercourse with his patients. Neither does it follow that, because it is contagious, it does not arise from common causes; its origin must have been once other than contagion, and we have no ground for supposing that it may not be occasionally so at the present time.

A few additional remarks may be made, tending to strengthen the opinion of contagion in puerperal fever.

It has been often noticed, that when the mothers have died of the prevalent epidemic, most, if not all, of the children have also been attacked with fatal peritoneal inflammation, as in the epidemic in the Philadelphia Hospital. Similar facts have been noticed at the Hospitals "de la Faculté" and "Maternité" of Paris.

Contagious diseases have many points of resemblance, which should not be lost sight of; one of the most striking is the remarkable insufficiency of all treatment, both rational and empirical. Examine the records of hospitals, and the reports of private practice, during most of the epidemics of malignant puerperal fever, and you will find upon every page the fearful truth, that it was possible to save but an exceedingly small portion of the patients—the loss of life in lying-in hospitals, according to Dr. Lee, more than counterbalances the benefits derived from them. This insufficiency of remedies shows that the grave forms of puerperal fever are something more than simple inflammations, to which many would reduce them; that, although in the course of an epidemic, there may be cases of an inflammatory character which will yield to a vigorous treatment, or the atmospheric constitution may give an inflammatory type to the great majority of cases, still the malignant form, whether inflammatory or not, whether sporadic or epidemic, is not arrested in its course by any treatment, as it depends on some element at present unknown to us. What is said by all in regard to variola, rubeola, typhus, &c., will equally apply to puerperal fever.

The contagious form differs also from the inflammations, which are often called puerperal fever, in that the symptoms and gravity of the affection are not in proportion to the extent or intensity of the accompanying inflammation; while in the latter the symptoms are generally the faithful expression, to a practised eye, of organic disorder. Insufficiency of remedies being peculiar to diseases capable of transmission, or, more properly speaking, these diseases always presenting this peculiarity, we may reasonably add this fact to the evidence in favour of the contagious nature of puerperal fever.

It is an interesting question whether puerperal fever ever attacks a second time; and if it does, whether this should be an argument against its contagious character. It is said that among acute diseases, those only which are contagious, are not produced a second time in the same individual; that there is no pure inflammation which cannot be reproduced several times: if, therefore, puerperal fever show itself more than once, that there is good reason to suppose it is not contagious. Here an important distinction may be made among contagious diseases: one class, in which may be placed small-pox, and the other exanthemata, generates a powerful virus which transmits the disease; the other, in which may be ranked typhus, puerperal fever, in which, though communicated by a morbific principle generated by them, different kinds of miasmata, or infection, may originate the disease, whose activity is increased by encumbered, ill-kept wards, and whose virulence is usually readily destroyed by separation, cleanliness, and ventilation. In the former, the disease is rarely originated, and the contagion is manifest; in the latter, the action of general causes often originates the affection, the contagion of which is less easily shown, from the very multiplicity of these causes.

We think the views of Professor Liebig tend to throw some light on this subject. This eminent chemist compares the action of certain poisons upon the animal economy to the process of fermentation. In this process, yeast (or oxydized gluten) undergoes putrefaction, and decomposes the sugar of the wort, which is converted into alcohol and carbonic acid; the gluten, also contained in the wort in large quantities, becomes oxydized, so that the original yeast seems to have been greatly multiplied. On the contrary, if the yeast had been placed in a solution of sugar free from gluten, the conversion of the sugar into alcohol and carbonic acid, would not have taken place in the same manner; but the yeast would have completely disappeared, instead of being reproduced. In small-pox, the poison converts some constituent of the blood into a matter similar to itself, and after its action has ceased, it leaves the system unsusceptible of the same action; and the poison can no more be reproduced than can yeast be formed in a solution of sugar, when the gluten is exhausted. From this he argues that the poison acted upon an element of the blood not absolutely essential to life, and which, when once removed, is not reproduced; thus explaining the action of those poisons which leave the system unsusceptible of a second attack. In this class of diseases, it is very rarely, if ever, that the poison is originated by any favourable combination of external circumstances; the virus acts continually, without generation "*de novo.*" In the second class, in which we place typhus, puerperal fever, erysipelas, &c., (most of which may attack an individual more than once,) the poison acts upon an element of the blood, which is essential to health, if not to life;

this element is constantly supplied, as yeast is continually formed, when gluten is added to the fermenting fluid, capable of causing a similar action in another solution of sugar. These diseases (the last two particularly, in which contagion is but an accidental element), are constantly being originated from a coincidence of favourable external circumstances and susceptible economies, and are afterwards propagated by contagion, till the virus loses more or less of its power from changes in the individuals, or their conditions—we should naturally expect to find them frequently reproduced, as the system is predisposed by the very circumstances in which they are originated. The immunity from second attacks is not, however, absolute in the first class of contagious diseases; may we not, then, reasonably suppose that, in the rare instances in which these are reproduced, the element of the solids, or the blood, upon which the poison spends its force, is also reproduced. [For more detail on the action of poisons, in this point of view, we may refer to the New York Lancet, for December 1842, p. 384.]

As every contagious disease has a period at which it is especially liable to be communicated, it may be asked at what time puerperal fever is most contagious. By a knowledge of this, many lives may perhaps be saved, or at least many well-founded fears quieted, by a timely separation of the affected, or the removal of the recently delivered from a suspected locality. Some diseases, as small-pox, are very contagious at their commencement, and far less so at their decline; others, as typhus, are most contagious at their decline. This point is not, so far as we can find, touched upon by writers on puerperal fever; but from the fatality attending communication with patients at all stages of the disease, from the fatal termination in from three to seven days, and the undoubted propagation from assisting at autopsies, we think we may conclude that it is more or less contagious from the commencement, according to the epidemic, and that this property goes on increasing with the duration of the affection; its latent period, if there be any, being so exceedingly short, as to announce an almost instantaneous action of the poison, and the reproduction of a similar one almost immediately.

An important question for the physician to ask himself is, how long a time should elapse, after attending a case of this disease, before he can safely visit a woman in labour. This is difficult to answer, as it is often almost impossible to know, in case a woman is seized with puerperal fever, (in the practice of a physician who has had other cases some time before,) whether she may not have been exposed to receive the contagion from another source than himself; and in many instances, sporadic cases in a single man's practice, have ceased for three or four weeks, when on attending his next case of midwifery, the patient has been attacked with the fever, no other case having occurred in the interim. Dr. Holmes concludes, that the physician should allow some weeks to elapse, if he has a single case of the disease, before he attends another woman; that if two cases follow each other in quick succession, when there are no cases in the neighbourhood, he should relinquish the practice of midwifery for at least one month, and endeavour by all possible means, to free himself from any noxious influence.

It does not appear that this contagion is conveyed to any great distance by the air. This seems indicated by the fact of the cases being confined to the practice of a single person in a large town, in whose crowded streets and lanes next door neighbours are not affected, unless visited by him; by the fact that seclusion affords almost complete security; by its being confined to single wards in large hospitals, &c., &c.

We will adduce, in conclusion, other authorities on the contagion of puerperal fever, which will strengthen, if possible, the evidence already brought forward.

Dr. Leake inclined to the belief that it depended on atmospheric changes; but he afterwards admitted that there might be an acquired contagion: exactly what we are contending for, viz:—that, though not necessarily contagious, it may become so.

From its prevalence in London, in 1787–8, Dr. Clarke, though he would not frankly allow its contagious character, confessed that its non-contagious nature could not be determined.

Dr. Denman admitted that it might sometimes be, at others not be, contagious.

Dr. James Hamilton, of Edinburgh, says, that the cause is a peculiar miasm; and that in certain atmospherical conditions it is contagious.

In the Dublin Lying-in-Hospital, the Edinburgh Infirmary, the Lying-in-Hospital, of Vienna; and during most of the London epidemics, it appears to have been propagated by contagion.

Hey, in speaking of the Leeds epidemic, seems to have strong doubts as to its non-contagiousness, and to be willing to allow a slight degree of contagion. Dr. Armstrong, who is opposed to its contagiousness, mentions that forty out of forty-three cases occurred in the practice of a single surgeon and his assistant, at Sunderland; this somewhat invalidates his opinion.

Dr. Hannay, of Glasgow, in his pamphlet on puerperal fever, observes, that many maintain that it is never communicated from one individual to another; while others refer nearly every case to contagion. He infers "that the disease may at its origin be sporadic and independent of contagion, and so generated become the source, where in epidemic rage, it may be communicated by contact to others."

Mr. Moore, on puerperal fever, remarks, "the facts bearing on the contagious nature of puerperal fever are numerous and forcible."

M. Voillemier, in speaking of the epidemic which raged at the Hôpital de la Faculté, in 1838, says, that it may be contagious; to which opinion, he adds, M. Dubois inclines. Dugès, and other French authors deny its contagious character; but, (independently of the little value of negative facts,) it was at a time when the contagious character of all diseases was there called in question.

In the Registrar General's Report, (*London Medical Gazette*, March, 1844,) it is stated that a certain number of deaths are caused by the contagion of this disease, communicated by the nurses and medical attendants.

Dr. Alison, (*Outlines of Pathology and Practice of Medicine,*) says, that puerperal fever is a "specific inflammation of the peritoneum, attending a contagious disease."

Mr. Ingleby, (*Edinburgh Medical and Surgical Journal,* 1838,) observes that, at Aylesbury, the contagiousness of puerperal fever was "as palpable as that of the small-pox." "Moreover, the great fatality of the disease at every period of its occurrence in the numerous lying-in-hospitals, both at home and abroad, admits of no other explanation."

Dr. Condie, (*American Journal*, above mentioned,) though not in general a contagionist, was convinced from the cases which he saw in the Philadelphia epidemic, that the puerperal fever at the time prevalent was capable of being communicated by contagion.

We have thus brought forward a mass of facts, (and many more might be adduced,) with the intention of showing that puerperal fever is frequently propagated by contagion. We have endeavoured to meet the objections made to this opinion, by showing that the same objections equally apply to many other diseases universally allowed to be contagious: though this does not solve the question at issue, but only postpones the difficulty, it may at least remove some stumbling-blocks from the path, placed there by our pretended guides, or left by the receding waters of ignorance.—We have seen that strict mathematical proof on this subject is impossible; until, then, the opponents of contagion can explain the phenomena of puerperal fever in a more probable or satisfactory manner than by our views, let them not reject these as unfounded and theoretical—nor, fixing their attention on some obscure point, require its full explanation before yielding a reluctant assent: this may be showing a praiseworthy unwillingness to allow the continuance of supposed dangerous error, but it is not certainly manifesting that candour and judgment which should animate the inquiries of the sincere lover of truth.

From the above series of facts, carefully collected and fairly stated, let every one reason for himself, and we think he will come to the same conclusions.

We think we may deduce the following propositions, from a careful examination.

1. From the confinement of cases to the practice of single physicians and nurses in populous cities; from the fatal results attending post-mortem examinations; from its ravages in hospitals; that puerperal fever is conta-

gious: that it may have other modes of propagation, in certain states of the atmosphere, and among strongly predisposed individuals; but that the fact of its conveyance by practitioners attests its contagiousness.

2. That it may be propagated by direct inoculation with the fluids of the living and the dead; by the effluvia arising from the bodies of the sick, inhaled in the very chamber of death, (as in the wards of a hospital,) or carried about by the person of the physician; by clothes, bedding, (fomites,) which have been in contact with a diseased individual.

3. That the order of propagation from the physician to the patient, and the regular succession of cases, show that the epidemics of puerperal fever are, in almost all cases, the *effects* and not the *causes* of the contagion.

4. The contagion acts according to the frequency of communication between the physician or nurse, (in whose practice are cases,) and lying-in women, independently of insalubrity of places, wretchedness of patients, or the neighbourhood of dwellings—for, although poverty and misery seem to predispose to it, communication is none the less fatal to the higher classes.

5. A case, to all appearance sporadic, may communicate the disease; a mild case may communicate a severe disease, and *vice versa*.

6. Immunity proves nothing against contagion; it may be the effect of an acquired or temporary inaptitude—it is equally inexplicable in all contagious diseases.

7. The rapidity of its propagation shows that it is contagious at the commencement; the fatal results of attending autopsies indicate this character after death.

8. That a physician should not make, or be present at an autopsy of this disease; or, if he does, should take proper measures to cleanse himself and dress, for the safety of his next patient—that if a case (or several cases) occur in his practice, he should consider himself, in the language of Dr. Holmes, "a private pestilence," and regulate his conduct accordingly—that persons who have washed, or have otherwise handled the clothes or bedding soiled by the discharges of this disease, should not approach, much less nurse, a woman after delivery.

9. That when the disease is prevalent, a prompt removal from possible intercourse with a "pestilential" physician, and a strict attention to ventilation, cleanliness, quiet, proper food, &c., are the dictates of a reasonable fear.

BOSTON, *November* 1845.